POLITICAL REGIME CHANGE, ECONOMIC REFORM AND GROWTH ACCELERATIONS

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Abstract

Using an improved definition and indicator of growth accelerations, we examine whether political regimes, regime changes, and economic reform are related to growth accelerations. Our results show that economic growth accelerations are preceded by economic reforms. Furthermore, we find that growth accelerations are more likely to happen after the start of a new political regime.

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1. Introduction

Various studies suggest that economic reform and the political regime in place are driving a country's economic growth performance.[1] However, models testing these relationships have been criticized for their limited ability to address causality (e.g. Durlauf, Johnson and Temple, 2005). Furthermore, cross-country growth regressions are based on very strong assumptions about a single linear model being appropriate for all countries in all states, while very few countries have experienced consistently constant growth rates over periods of several decades. Therefore, Hausmann, Pritchett and Rodrik (2005) focus on so-called growth accelerations and examine which covariates are related to periods of rapid economic growth.[2] They find that growth accelerations are often preceded by political regime changes, while economic reform is hardly related to these accelerations.[3]

In this paper we re-examine the relationship between economic reform, political regime changes, and growth accelerations, making the following contributions. First, we improve upon the filter used by Hausmann *et al*. to identify growth accelerations as this filter yields some strange outcomes. For example, 27 episodes identified as accelerations have higher growth in the year before the acceleration than at the start of the acceleration. Second, we find that country fixed effects should not be neglected when examining growth accelerations. Therefore, we use conditional fixed effects logistic regressions as proposed by Chamberlain (1980) instead of (pooled) probit regressions. Third, we do not only focus on (the direction of) regime changes, but also examine the role of regime duration. Finally, we do not solely rely on the Polity IV dataset, but also investigate the robustness of our results using alternative regime indicators.

Our main findings are that that economic growth accelerations are preceded by economic reforms. Furthermore, we find that growth accelerations are more likely to happen after the start of a new political regime.

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[1] For a survey on the relationship between economic growth and political regimes see e.g. Przeworski, Alvarez, Cheibub and Limongi (2000). Winters (2004) provides an overview of the relationship between trade liberalization and economic growth, while the literature on economic reform and economic growth is surveyed by De Haan, Lundström and Sturm (2006).

[2] Related papers that organise the data around particular events to identify causal determinants of growth include Jones and Olken (2005a, b).

[3] However, Jong-A-Pin and De Haan (2006) argue that these results are driven by an error in the dataset of Hausmann *et a*l. and find that regime changes are not related to growth accelerations.

The paper proceeds as follows. Section 2 discusses our filter to identify economic growth accelerations. Section 3 analyzes the relationship between political regime changes, economic reform and growth accelerations, while section 4 examines the robustness of our results. Section 5 concludes.

2. Identifying growth accelerations

For the period 1957-1992, Hausmann *et al.* (2005) identify no less than 83 periods of accelerated growth, using the following filter. For each country (with more than 1 million inhabitants and more than 20 available observations), the logarithm of real GDP per capita (taken from the Penn World Tables 6.2.) is regressed on time for every eight year period (n=7). That is,

$$\ln(y_{t+i}) = a + g_{t,t+n} * t + \varepsilon, \qquad i = 0, \cdots, n$$

Where *y* denotes real GDP capita and *t* is time. The estimated parameter, $g_{t,t+n}$, is taken as a proxy for the average growth rate over the period *t* to *t+n* and labeled the "least squares growth rate". To qualify as a growth acceleration, the least squares growth rate should be at least 3.5% per annum. Furthermore, it should be at least 2 percentage points higher than in the previous eight years. Finally, to rule out episodes of full economic recovery, the level of real GDP should be higher at the end of the acceleration than in all years before the acceleration. In cases that consecutive years qualify to be the start of a growth acceleration, the year is chosen with the highest F-statistic of a piecewise linear (or spline) regression with the break at the relevant year. Hausmann *et al.* allow for the possibility that an acceleration is followed by another acceleration as long as the second acceleration starts at least five years after the first one.

Hausmann *et al.* (2005) provide several robustness checks to convince the reader that the chosen criteria are defensible. Still, we argue that these criteria can be improved upon. Table 1 reports the growth rate before the start of the growth accelerations identified by Hausmann *et al*, the growth rate in the first year of the acceleration and the eight year growth average according to data from the Penn World Tables 6.2.

[Table 1 here]

It follows that 18 growth acceleration episodes as identified by Hausmann *et al*. have a negative growth rate in the first year of the acceleration. Furthermore, 27 of the identified episodes have a higher growth rate in the year before the acceleration than at the start of the acceleration. Finally, in 10 episodes, growth in the year before the acceleration is higher than the average growth rate during the entire acceleration. On the basis of these figures it seems that episodes falling in the first category are identified too early, while episodes falling in the latter categories are identified too late.[4]

To identify the initiation of a growth acceleration adequately, we propose a simple criterion that first identifies all possible take-offs of an acceleration. This criterion is similar to the so-called BBQ rule used to identify turning points in business cycles (Harding and Pagan, 2002). To qualify as the start of an economic growth acceleration in year *t*, growth should be higher than in the year before[5]:

$$g_{t+1} > g_t$$

If year *t* fulfills this criterion, we check whether it also fulfills the three criteria of Hausmann *et al.* (2005):

$$g_{t,n+7} > 3.5 \ ppa$$
$$g_{t,n+7} - g_{t-7,t} > 2.0 \ ppa$$
$$y_{t+7} > \max\{y_i\} \quad i \le t$$

If these are satisfied as well and year *t-1* and year *t+1* do not qualify, year *t* is the start of the growth acceleration. If consecutive years fulfill all criteria, we take the year in which the level of real GDP is minimal to be the starting point of the growth acceleration. This

[4] In our opinion, the reason why the filter is unable to identify the start of the growth acceleration adequately is the use of the spline regression method. If one break is allowed in the fitted function, its optimal position is also affected by the volatility and other breaks in the series. As a consequence, the criterion of the highest F-statistic of the fitted function gives the best fitted line through the GDP series, but not necessarily the optimal location of the break.

[5] As a robustness check, we also examined the criterion that the two-year growth rate should exceed the two-year growth rate before the start of the growth acceleration. This alternative criterion ensures that the growth rate at the start of the acceleration is substantial.

simple rule ensures that the growth rate in the first year of an acceleration *always* increases relative to the previous year. Our filter identifies 84 accelerations, which are reported in Table 2.[6]

[Table 2 here]

There are substantial differences between accelerations based on the filter of Hausmann *et al*. (2005) and those based on our filter. Only 28 episodes (33%) are identified at exactly the same date, 31 episodes (37%) differ one year, while 15 episodes (18%) are identified with more than one year difference. Furthermore, our filter does not pick up 9 episodes, which are identified by the filter of Hausmann *et al*. as these episodes do not fulfill the condition that the growth rate at the first year of the acceleration should be larger than the growth rate in the year before the acceleration.[7] Finally, we identify 10 episodes not found by Hausmann *et al*..

To illustrate the different results, Figure 1 shows the real GDP series of Chile, Botswana and Mali.

[Figure 1 here]

The figure indicates in which year the growth acceleration starts according to both approaches. It can be seen that in the case of Chile, the timing of the filter of Hausmann *et al*. is too late, while it is too early in the case of Botswana. The case of Mali illustrates that the acceleration identified by Hausmann *et al*. in fact lasts only 6 years. However, it is identified as an acceleration, because the average growth rate in the last 6 years of the acceleration compensate for the negative growth rate in the first two years of the

[6] We checked whether our episodes suffer from the same problem as dealt with in Table 1. In turns out that on two occasions our filter identifies an acceleration which is preceded by a year that has a higher growth rate than the average growth rate during the entire acceleration: Congo (1969) and Pakistan (1965). In the case of Congo, the actual growth acceleration started before the sample period, i.e. 1967. The situation of Pakistan is different as 1965 is the first possible start after the previous acceleration.

[7] These episodes are (starting year in parenthesis): Guinea-Bissau (1988), Haiti (1990), Malawi (1992), Mali (1972), Papua New Guinea (1987), Poland (1992), Syria (1974), Uganda (1977) and Uruguay (1989).

acceleration. As it is required that the acceleration has to be (at least) 8 years, our rule makes sure that these quasi growth accelerations are not identified.

3. Empirical Analysis

We examine which variables trigger the start of a growth acceleration. In particular, we are interested in the effect of economic reform and political regime changes. The dependent variable we employ in the analysis takes a value of one centered on the timing of the start of a growth acceleration and zero otherwise.[8] Our dataset consists of 106 countries over the period 1957-1993 of which 57 countries experienced at least one growth acceleration.

As a proxy for economic reform we rely on the index provided by Wacziarg and Welch (2003), that incorporates a number of structural features (e.g., presence of marketing boards and socialist economic regimes) and the macroeconomic environment (e.g., presence of a large black-market premium for foreign currency), in addition to tariff and non-tariff barriers to trade. Following Hausmann *et al*. (2005), the variable included is a dummy that takes the value of 1 during the first five years of a transition towards "openness".

Our regime change variable is taken from the Polity IV dataset of Marshall and Jaggers (2002). A regime change is defined as a three points change in the Polity score in three years or less. However, as it is possible that a country has no formal regime for a couple of years, we focus only on those changes, which are followed by a continuation of the same regime for at least one year. Like the economic reform dummy, we use a dummy equal to one for the first five years after a political regime change and zero otherwise. Following Hausmann *et al.* (2005), we differentiate between positive (i.e. changes towards more democracy) and negative regime changes (changes towards more autocracy). In addition, we add the durability of a regime defined as the number of years

[8] We follow Hausmann *et al.* regarding the treatment of the dependent variable. That is, if an acceleration starts in year *t* then year *t-1* and *t+1* also are assigned the value one. As shown in the previous section, there is some uncertainty concerning the timing of a growth acceleration and following the approach of Hausmann *et al*. reduces the probability of missing the initiation of an acceleration. Furthermore, we omit those observations from the analysis which cannot be the start of the acceleration (years *t+2 – t+5*).

that the current regime is in place, following the suggestion of Clague, Keefer, Knack and Olson (1996).[9]

We test for the appropriate panel data model using the Hausman test (Hausman, 1978). We test the null-hypothesis that all country fixed effects equal zero by comparing the estimates of a conditional fixed effects logit (CFEL) model (see Chamberlain, 1980) and the unrestricted (pooled) logit model. The null-hypothesis of no country specific effects is rejected for all model specifications. Hence, CFEL should be preferred. Furthermore, we examine the presence of time effects in the dataset. Figure 2 shows the average number of growth accelerations per year for the entire sample period. The pattern we roughly observe is that the number of growth accelerations is steadily decreasing over time. Therefore, we include a linear trend in our model, which turns out to be highly significant in all specifications.

[Figure 2 here]

Table 3 contains the estimation results. The results reported in columns 1-4 are based on the full sample, while columns 5-8 excludes those episodes that started just after an economic crisis. As it seems likely that economic growth picks up after a crisis, we want to make sure that our results are not driven by the inclusion of this kind of 'economic recovery' accelerations. In case the growth rate in the two years before the growth acceleration is smaller than -10% we define it as a post-crises growth acceleration.[10]

[Table 3 here]

It can be immediately observed that the results for the two samples are very similar. In contrast to the results reported by Hausmann *et al*. (2005), we find that the effect of economic reform on the probability of a growth acceleration is highly significant in all specifications. However, the results for political regime changes are less clear. Political regime changes are in general not related to growth accelerations, but we do find a

[9] The correlation coefficient between our regime change dummy and the durability indicator is -0.37.
[10] Using a threshold of -5% results in exactly the same result (14 post crisis growth accelerations). If a threshold of -15% is used, we find only 3 such episodes.

negative and significant effect of regime duration for all specifications. This implies that growth accelerations are more likely to happen after the start of a new political regime. Although the results seems to contradict each other, we think this can be explained by the fact that our regime change dummy only takes the value of one in the first five years after a regime change. It can therefore be concluded that for a growth acceleration to happen, it may take more than five years after a political regime change.

If we differentiate between regime changes towards more democracy and changes towards more autocracy, we find that regime changes are only marginally related to the initiation of a growth acceleration. The point estimates, however, suggest that growth accelerations are more likely to happen in the first five year after a change towards more autocracy than a change towards more democracy.

4. Robustness analysis

To check the robustness of our results we run several variants of our base model. The results are reported in Table 4.[11]

[Table 4 here]

First, we replace the regime change variable for two alternative indicators. These alternative indicators are based on Prezworski *et al*. (2000) and Vanhanen (2000). We define a regime change if according to these authors the regime switches from democracy to autocracy and vice versa. According to both measures, a change to more autocracy significantly increases the probability of a growth acceleration. A change to more democracy is marginally related to growth accelerations according to the indicator of Vanhanen.

Next, we examine the robustness of our results using a variant of our filter. Instead of the rule that in the first year of the acceleration the growth rate has to be higher than in the year before the acceleration, we now restrict the filter to identify those take-

[11] The results presented here are based on the sample including all episodes. We also checked whether the results differ if we exclude accelerations after an economic crisis. These results are very similar to the results presented here and are available on request.

offs in which the two-year growth rate is higher than the two-year growth rate before the start of the acceleration. Again, the results are very similar to those presented in the baseline model.

Finally, we add several control variables to the baseline model. These variables are also used by Hausmann *et al*. (2005). In column 6 we incorporate terms of trade, whilst we add a dummy for financial liberalization in column 7. Unfortunately, the use of additional control variables decreases the sample size. It can be seen that adding the terms of trade variable or the financial liberalization measure affects the result for the economic reform variable, which becomes insignificant. This result is driven by the fact that a number of countries drop from the sample, which had an economic reform just before a growth acceleration.[12]

When we include variables that capture characteristics of the political environment such as the tenure and death of a political leader (following Jones and Olken (2005b)), or the presence (or end of) a civil war, we obtain the same results as in the baseline model.

5. Concluding comments

Due to the fixation on long-run differences in growth, empirical growth research has underestimated the importance of instability and volatility in growth rates, especially in developing countries. Traditional cross-country and panel growth models are not well suited to use information provided by the time dimension. Periods chosen in panel models are often justified only on the grounds that data were available at those frequencies or the researcher wanted to divide the whole period into equal chunks. These periods are unlikely to identify information provided by the development of variables over time. If, for instance, there is a high growth rate in the first five years and a low growth rate in the second half of the ten year period, the period average will be rather uninformative.

[12] In fact, the four countries that drop from the sample if we include financial liberalisation all had an economic reform just before the start of the acceleration, i.e. Denmark, Tunisia, Taiwan and Uganda.

The approach suggested by Hausman *et al*. (2005) is much more promising to use the information provided by the time dimension. These authors focus on turning points in growth performance, examining instances of rapid acceleration in economic growth that are sustained. They identify more than 80 such episodes since the 1950s, finding that political-regime changes are statistically significant predictors of growth accelerations.

Building upon the work by Hausman et al., we make various contributions. First, we improve upon the filter used by Hausmann *et al*. to identify growth accelerations as their filter yields some strange outcomes. Second, we test for country fixed effects and based on the testing outcomes use conditional fixed effects logistic regressions as proposed by Chamberlain (1980) instead of (pooled) probit regressions. Third, we do not only focus on (the direction of) regime changes, but also examine the role of regime duration. Finally, we investigate the robustness of our results using alternative regime indicators.

Our main findings differ from those of Hausmann *et al*. (2005). First, we find that economic growth accelerations are preceded by economic reforms. Furthermore, our results suggest the impact of political regimes changes is not robust, but that growth accelerations are more likely to happen after the start of a new political regime.

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Table 1. Growth accelerations according to Hausmann *et al*. (2005)

Country	*(1) Year*	*(2) Growth in year before acceleration*	*(3) Growth at start acceleration*	*(4) Average growth during acceleration*	*(5) Growth before> growth at start?*	*(6) Growth before>growth during?*	*(7) Growth at start <0?*
Algeria	1975	1.6	10.7	4.2	n	n	n
Argentina	1963	-6.1	7.7	3.6	n	n	n
Argentina	1990	-6.0	9.8	6.1	n	n	n
Australia	1961	-1.7	4.8	3.8	n	n	n
Belgium	1959	3.8	5.4	4.5	n	n	n
Botswana	1969	5.1	-5.1	11.7	y	n	y
Brazil	1967	2.5	8.4	7.8	n	n	n
Canada	1962	5.1	3.4	3.6	y	y	n
Chad	1973	-8.3	-4.6	7.3	n	n	y
Chile	1986	3.1	3.7	5.5	n	n	n
China	1978	-1.1	10.6	6.7	n	n	n
China	1990	7.0	10.6	8.0	n	n	n
Colombia	1967	0.8	2.9	4.0	n	n	n
Comoros	1972	-2.8	0.6	5.3	n	n	n
Congo Rep.	1969	9.0	12.2	5.4	n	y	n
Congo Rep.	1978	7.9	13.7	8.2	n	n	n
Denmark	1957	4.0	0.5	5.3	y	n	n
Dominican Rep.	1969	5.4	13.3	5.5	n	n	n
Dominican Rep.	1992	4.2	3.8	6.3	y	n	n
Ecuador	1970	4.2	2.9	8.4	y	n	n
Egypt	1976	10.3	9.8	4.7	y	y	n
Finland	1958	-2.1	7.1	5.0	n	n	n
Finland	1967	1.3	0.6	5.6	y	n	n
Finland	1992	-7.8	-4.3	2.8	n	n	y
Ghana	1965	-21.7	22.7	8.3	n	n	n
Guinea-Bissau	1969	-1.7	-27.8	8.1	y	n	y
Guinea-Bissau	1988	-5.4	-1.6	5.2	n	n	y
Haiti	1990	1.9	11.6	12.7	n	n	n
India	1982	3.2	5.3	3.9	n	n	n
Indonesia	1967	-1.6	9.4	5.5	n	n	n
Indonesia	1987	2.5	4.0	5.5	n	n	n
Ireland	1958	0.0	4.8	3.7	n	n	n
Ireland	1985	2.5	-0.6	5.0	y	n	y
Israel	1957	3.6	3.2	5.3	y	n	n
Israel	1967	-6.0	17.2	7.2	n	n	n
Japan	1958	3.4	8.4	9.0	n	n	n
Jordan	1973	-1.5	-1.7	9.1	y	n	y
Rep. Korea	1962	-0.4	8.9	6.9	n	n	n
Rep. Korea	1984	7.2	5.3	8.0	y	n	n
Lesotho	1971	3.4	-0.6	5.3	y	n	y
Malawi	1970	-9.1	21.8	3.9	n	n	n
Malawi	1992	-10.3	12.8	4.8	n	n	n
Malaysia	1970	3.3	3.2	5.1	y	n	n
Malaysia	1988	6.2	6.4	5.7	n	y	n

Mali	1972	2.8	-3.6	3.8	y	n	y
Mauritius	1971	-0.5	4.4	6.7	n	n	n
Mauritius	1983	-0.2	2.6	5.5	n	n	n
Morocco	1958	5.4	1.3	7.7	y	n	n
New Zealand	1957	3.4	-1.3	3.8	y	n	y
Nicaragua	1960	6.3	3.6	4.8	y	y	n
Nigeria	1957	-0.6	4.2	4.3	n	n	n
Nigeria	1967	-17.8	-3.2	7.3	n	n	y
Norway	1991	2.5	2.5	3.7	n	n	n
Pakistan	1962	1.7	4.0	4.8	n	n	n
Pakistan	1979	-1.7	7.3	4.6	n	n	n
Panama	1959	-0.7	9.7	5.4	n	n	n
Panama	1975	1.2	-0.6	5.3	y	n	y
Papua New Guinea	1987	0.0	-4.1	4.0	y	n	y
Paraguay	1974	3.9	4.3	6.2	n	n	n
Peru	1959	-6.0	10.6	5.2	n	n	n
Poland	1992	1.9	3.5	5.0	n	n	n
Portugal	1985	2.7	4.3	5.4	n	n	n
Romania	1979	6.5	-2.1	12.4	y	n	y
Rwanda	1975	-2.1	15.1	4.0	n	n	n
Singapore	1969	12.5	12.3	8.2	y	y	n
Spain	1959	-4.5	8.2	8.0	n	n	n
Spain	1984	0.7	1.0	3.8	n	n	n
Sri Lanka	1979	0.5	1.1	4.1	n	n	n
Syria	1969	11.4	-19.2	5.8	y	y	y
Syria	1974	5.2	17.1	4.8	n	y	n
Syria	1989	-10.0	6.3	4.4	n	n	n
Taiwan	1961	4.0	4.6	7.1	n	n	n
Thailand	1957	10.9	-0.6	5.3	y	y	y
Thailand	1986	3.0	7.2	8.1	n	n	n
Trinidad &Tobago	1975	1.4	8.5	5.4	n	n	n
Tunisia	1968	9.7	4.7	6.6	y	y	n
Uganda	1977	-0.8	-6.3	4.0	y	n	y
Uganda	1989	1.3	5.2	3.6	n	n	n
United Kingdom	1982	2.4	3.8	3.5	n	n	n
United States	1961	0.4	4.7	3.9	n	n	n
Uruguay	1974	1.8	5.0	4.0	n	n	n
Uruguay	1989	0.1	-0.9	3.8	y	n	y
Zimbabwe	1964	5.6	10.1	7.2	n	n	n

Notes: column (1) indicates the start of the acceleration; column (2) shows the rate of growth in the year preceding the acceleration; column (3) contains the growth rate at the start of the acceleration; column (4) presents the average growth rate during the episode; column (5) indicates whether the growth before the start of the acceleration exceeds the growth rate at the start of the episode (y = yes; n = no); column (6) indicates whether the growth before the start of the acceleration exceeds the average growth rate of the episode; and column (7) shows whether there is a negative growth rate at the start of the episode.

Table 2. Growth accelerations according to our filter

Country	*(1) Year*	*(2) Growth before*	*(3) Growth during*	*(4) Difference in growth rate*	*(5) Difference in years with HPR*
Algeria	1975	2.1	4.2	2.1	0
Argentina	1963	0.9	3.6	2.7	0
Argentina	1990	-3.1	6.1	9.2	0
Australia	1961	1.5	3.8	2.3	0
Belgium	1958	2.4	4.7	2.3	1
Botswana	1970	3.3	11.4	8.1	1
Brazil	1966	3.1	7.2	4.1	1
Cameroon	1972	-0.6	5.3	5.9	0
Canada	1961	1.0	3.8	2.8	1
Chad	1974	-1.5	7.3	8.8	1
Chile	1983	1.7	4.2	2.5	3
Chile	1988	1.0	5.8	4.8	ni5
China	1976	2.0	5.3	3.4	2
China	1981	3.5	6.5	3.0	ni5
China	1989	5.2	8.7	3.5	1
Colombia	1967	1.6	4.0	2.4	0
Congo Rep.	1969	0.9	5.4	4.5	0
Congo Rep.	1977	3.8	8.7	4.9	1
Denmark	1958	2.2	5.2	3.0	1
Dominican Rep.	1968	-0.2	6.5	6.6	1
Dominican Rep.	1991	0.4	5.8	5.3	1
Ecuador	1966	1.3	4.6	3.3	4
Ecuador	1971	1.6	7.7	6.1	ni5
Egypt	1975	-1.1	5.5	6.7	1
Finland	1958	2.7	5.0	2.2	0
Finland	1968	2.8	5.3	2.4	1
Finland	1993	-0.4	4.4	4.8	1
Ghana	1965	-0.1	8.3	8.4	0
Guinea-Bissau	1971	-5.5	10.0	15.4	2
India	1982	1.5	3.9	2.4	0
Indonesia	1967	-0.8	5.5	6.2	0
Indonesia	1987	3.4	5.5	2.1	0
Ireland	1957	1.5	3.8	2.4	1
Ireland	1986	1.5	4.9	3.5	1
Ireland	1993	4.9	8.2	3.2	ni93
Israel	1958	2.4	5.4	2.9	1
Israel	1967	2.8	7.2	4.4	0
Japan	1958	5.8	9.0	3.2	0
Jordan	1971	-3.1	6.0	9.1	2
Rep. Korea	1960	1.9	4.9	3.0	2
Rep. Korea	1965	2.6	7.4	4.8	ni5
Rep. Korea	1981	5.5	7.8	2.3	3
Lesotho	1969	1.7	3.9	2.2	2
Malawi	1970	1.5	3.9	2.5	0
Malaysia	1971	3.0	5.0	2.1	1
Malaysia	1986	2.3	5.5	3.2	2

Mali	1993	0.2	3.7	3.6	ni93
Mauritius	1969	-1.5	4.7	6.2	2
Mauritius	1983	1.0	5.5	4.4	0
Morocco	1957	-0.9	7.8	8.7	1
New Zealand	1958	2.1	4.1	2.0	1
Nicaragua	1959	1.1	5.2	4.1	1
Nigeria	1957	1.2	4.3	3.0	0
Nigeria	1968	-3.4	4.6	8.0	1
Norway	1991	1.4	3.6	2.2	0
Pakistan	1960	-1.8	4.3	6.0	2
Pakistan	1965	0.7	3.5	2.9	ni5
Pakistan	1977	1.6	4.4	2.7	2
Panama	1959	1.5	5.4	3.9	0
Panama	1976	1.9	5.3	3.4	1
Paraguay	1973	2.6	5.9	3.3	1
Peru	1959	0.8	5.2	4.4	0
Portugal	1984	1.6	5.6	4.0	1
Romania	1980	0.6	14.5	13.8	1
Rwanda	1975	0.7	4.0	3.3	0
Singapore	1967	7.0	10.6	3.6	2
Spain	1959	4.4	8.0	3.5	0
Spain	1984	0.1	3.8	3.7	0
Sri Lanka	1979	1.9	4.1	2.2	0
Syria	1970	-0.3	5.2	5.5	1
Syria	1989	-2.9	4.4	7.3	0
Taiwan	1960	3.3	6.8	3.4	1
Taiwan	1966	6.1	8.6	2.5	ni5
Thailand	1958	-0.9	5.4	6.3	1
Thailand	1983	4.3	6.6	2.3	3
Trinidad &Tobago	1971	2.1	4.8	2.7	4
Tunisia	1969	2.5	6.4	3.8	1
Tunisia	1993	1.6	3.7	2.1	ni93
Uganda	1989	-0.8	3.6	4.4	0
United Kingdom	1982	1.1	3.5	2.5	0
United States	1961	0.9	3.9	3.0	1
Uruguay	1974	1.5	4.0	2.6	0
Zimbabwe	1963	0.2	6.6	6.4	1
Zimbabwe	1968	3.5	6.5	3.0	ni5

Notes: column (1) indicates the start of the acceleration; column (2) shows the rate of growth in the 8 years preceding the acceleration; column (3) presents the average growth rate during the episode; column (4) presents the difference between column (2) and (3); and column (5) shows the differences between our filter and that of Hausmann *et a*l. (2005). Ni5 denotes that the fact that we identify this acceleration, whilst Hausmann et al. (2005) do not identify this acceleration. This is due to a timing difference of the first acceleration and the possibility of an acceleration from the 5th year onwards after the first acceleration. Ni93 denotes accelerations starting in 1993. These are not included by Hausmann et al. (2005).

Table 3. Estimation results: baseline model

Dependent variable: timing of a growth acceleration	*all episodes*				*post crises periods excluded*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Trend	-0.066	-0.052	-0.056	-0.062	-0.067	-0.049	-0.051	-0.063
	(7.72)***	(5.41)***	(6.08)***	(7.14)***	(7.25)***	(4.67)***	(5.12)***	(6.65)***
Economic Reform	0.811	0.724	0.752	0.803	1.061	0.967	0.982	1.052
	(2.85)***	(2.53)**	(2.66)***	(2.82)***	(3.47)***	(3.15)***	(3.22)***	(3.44)***
Regime change	-0.117	-0.491			0.255	-0.257		
	(0.47)	(1.82)*			(0.96)	(0.89)		
Regime duration		-0.037	-0.027			-0.048	-0.042	
		(3.08)***	(2.53)**			(3.55)***	(3.59)***	
Positive regime change				-0.645				-0.248
				(1.92)*				(0.72)
Negative regime change				0.398				0.824
				(1.30)				(2.45)**
Pseudo R-squared	0.08	0.09	0.09	0.09	0.09	0.11	0.11	0.10
Observations	1235	1235	1238	1235	1059	1059	1061	1059
Countries	46	46	46	46	40	40	40	40
Hausman test, prob>Chi^2	0.0028	0.0002	0.0039	0.0067	0.0129	0.0001	0.0001	0.0097

Absolute value of z statistics in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

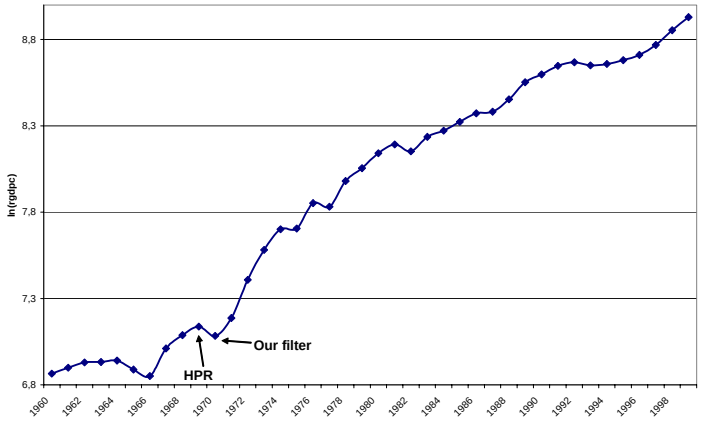
Table 4. Sensitivity analysis

Dependent variable: timing of a growth acceleration	*Regime changes according to Vanhanen*		*Regime changes according to Przworski et al.*		*2year filter*	*Various control variables added*			
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*	*(9)*
Trend	-0.054	-0.049	-0.053	-0.049	-0.036	0.012	-0.036	-0.032	-0.039
	(5.64)***	(5.01)***	(5.44)***	(4.98)***	(3.47)***	(0.65)	(2.94)***	(2.80)***	(3.45)***
Economic reform	0.726	0.717	0.589	0.536	0.356	0.101	0.434	0.687	0.725
	(2.56)**	(2.52)**	(1.99)**	(1.81)*	(1.12)	(0.25)	(1.29)	(2.13)**	(2.26)**
Regime change	0.064		0.241						
	(0.23)		(0.86)						
DURABLE	-0.028	-0.031	-0.026	-0.029	-0.044	-0.053	-0.056	-0.057	-0.050
	(2.58)***	(2.80)***	(2.27)**	(2.47)**	(3.54)***	(2.60)***	(4.09)***	(4.24)***	(3.79)***
Positive regime change		-0.763		-0.205	-1.234	-1.879	-1.958	-1.796	-1.784
		(1.91)*		(0.56)	(3.45)***	(3.39)***	(4.03)***	(4.10)***	(4.04)***
Negative regime change		0.727		0.682	0.195	0.226	-0.008	-0.052	-0.047
		(2.26)**		(1.99)**	(0.64)	(0.52)	(0.02)	(0.16)	(0.14)
Terms of trade						0.014			
						(0.03)			
Civil war									1.092
									(1.92)*
End of war									-0.341
									(0.98)
Leader death								-0.267	
								(0.44)	
Tenure death								-0.097	
								(1.29)	
Financial liberalization							0.588		
							(1.24)		
Observations	1215	1215	1208	1208	1239	559	1035	1121	1121
Countries	45	45	45	45	46	29	42	42	42
Pseudo R-squared	0.08	0.10	0.08	0.08	0.09	0.05	0.11	0.12	0.11

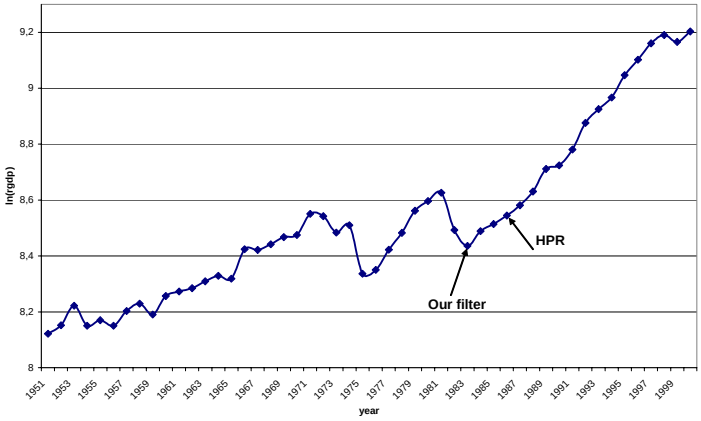
Absolute value of z statistics in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

Figure 1. Identifying growth accelerations

Botswana



Chile



Mali

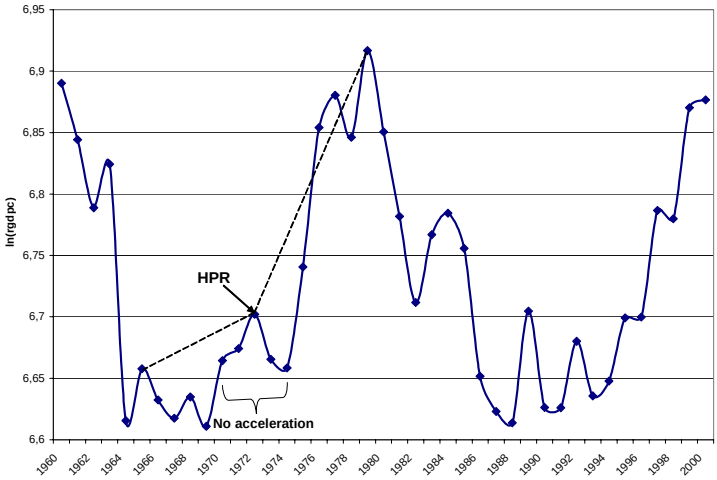
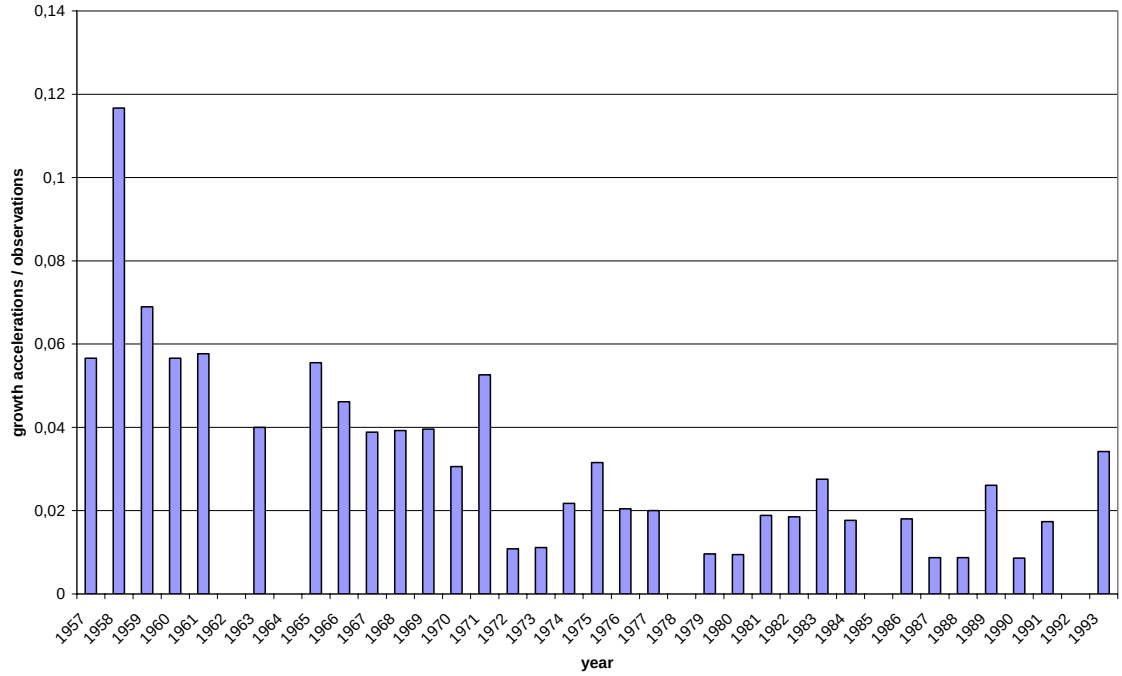


Figure 2. Growth accelerations over time, 1957-1993



Note: the figure shows the number of growth accelerations per year.

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